

## CORRESPONDENCE.

[Correspondents are requested to write briefly and to the point. No attention will be paid to anonymous communications.]

## Propatagialis cucullaris.

TO THE EDITORS OF THE AUK:—

Dear Sirs:—Having given space in 'The Auk' to Dr. Shufeldt's rejoinder to an article of mine in another publication, which probably few of the readers of 'The Auk' have had the opportunity to familiarize themselves with, will you kindly allow me to say a few words in my defense, the more so, as Dr. Shufeldt has told this new class of readers that my first paper was "a rather acrimonious protest" in which I "so misrepresented the entire matter," etc. I regret very much that the editor of 'Science' did not think Dr. Shufeldt's reply fit for publication, since had it appeared in that journal, I should have saved myself the trouble of answering his irrelevant rejoinders and counter-criticisms. The readers of 'Science' who knew the previous articles would also know how to correctly place his reply, and would be competent judges whether I had "misrepresented the whole matter" or not. It is also characteristic that Dr. Shufeldt did not make this accusation in the reply intended for 'Science,' but in the part prepared for 'The Auk' only. To this accusation I can only say, read the original articles and judge! In every instance I quoted Dr. Shufeldt *verbatim*. Besides there was no room for misrepresentation.

The whole sum and substance of the controversy is this: In 'Science' for June 24, 1887, Dr. Shufeldt announced what he took to be the discovery of an unknown muscle in the bird's wing, which he thought without a name, and which he therefore named *dermo-tensor patagii*, alleging that it had a special taxonomic value. My article in 'Science' for August 5, 1887, demonstrated that Dr. Shufeldt was entirely wrong in all his suppositions. I proved that this muscle was not confined to the *Passeres acromyodi*, but that it is equally well developed in Parrots and Woodpeckers; I proved that the muscle, so far from being unknown and unnamed, was well known in literature, and had not one but many names; and I proved that Dr. Shufeldt's allegation that the late Professor Garrod in particular was ignorant of the existence of this muscle, was equally unfounded.

I did not blame Dr. Shufeldt for not knowing these things, and, surely, I did not exhibit any "acrimony." I did not feel any then, and I do not feel any now. I only stated scientific facts, killed a false notion at its birth, and assigned '*dermo-tensor patagii*' to the limbo of synonyms. That was my entire crime!

I repeat, I did not blame Dr. Shufeldt for not knowing the literature

on this point, in fact, I did not then blame him at all, for I knew very well the disadvantages under which he labors, and which he justly pleads as extenuating circumstances. But when a student knows these difficulties himself, he has no excuse for rushing into print with his so-called discovery because he does not find this small muscle mentioned in a few English works, either too general or too special for the purpose. There was no need of hurrying the publication of such half-digested matter; if Dr. Shufeldt had inquired from one of his many correspondents who had access to the literature, and had postponed the heralding of the discovery until its importance had been confirmed, he might have saved himself considerable trouble and the mortification of a correction.

Now only a few words in reply to Dr. Shufeldt's letter in 'The Auk' (1887, pp. 353-356), and in order to be brief and to avoid repetitions, I shall take up his points *seriatim*.

It is curious to hear Dr. Shufeldt call the authorities whom I quoted "dissectors, as a rule, who did not especially look into the structure of the birds with the view of determining their affinities." Now the fact is quite the reverse, and by his remark Dr. Shufeldt clearly proves that he does not know these men, nor their works. It is sufficient to state that most of them are comparative systematists whose aims and achievements in this latter direction make Dr. Shufeldt appear a mere "dissector" by comparison.

Dr. Shufeldt in speaking of my defense of Professor Garrod says: "I am, as it were, directly charged with doing Professor Garrod a 'great injustice', and 'gravely misrepresenting' him, *as if* that were the *sole aim* of my original description" (italics mine). Suffice it to say that the "*as if*" is a pure insinuation. I have made no such allusion nor have I hinted at Dr. Shufeldt's *aim*. There is not a word to indicate that I thought Dr. Shufeldt misrepresented Garrod willingly or knowingly. He did misrepresent him nevertheless.

That Dr. Shufeldt failed to find a trace of *propatagialis cucullaris* in two specimens of *Tyrannus tyrannus* while I myself discovered distinct muscular elements, shows very plainly the unstable character and comparative unimportance of this muscular slip.

We now come to the second half of Dr. Shufeldt's reply, which may safely be characterized as an attempt to raise sufficient dust to conceal the real questions at issue, for he takes nearly a whole page of the valuable space of 'The Auk' to criticise such parts of my drawings as have no bearing upon the discussion. But as he has raised these side-issues, and finally comes back to them in the finishing paragraph of his reply with a somewhat supercilious allusion, I am obliged to ask some space in order to demonstrate how utterly devoid of foundation his allegations are. First he makes some remarks in regard to the scale to which my figures were stated to have been drawn, viz., one third natural size. Any "intelligent" reader will at once see that this statement is due to a clerical, or a typographical error. I received no proof of the figures illustrating my original article nor of the explanatory text accompanying them. Of course when I

saw the number of 'Science' containing them I immediately discovered the lapsus, but I had sufficient confidence in the readers of that journal to believe that not a single one of them could be deceived by it, and consequently I deemed it unnecessary to formally correct such a trifling matter. The original drawings were natural size, and on the paper I marked them to be reduced to one third, hence, of course, the mistake. But I will here emphasize that this is the *second time* that Dr. Shufeldt, in a controversy with me in this journal, has taken advantage of an *obvious* error of this kind. There are at least half a dozen other typographical errors in that paper of mine, for my return proofs evidently did not reach the printer in time, and it is only a matter of surprise to me that Dr. Shufeldt did not avail himself of the opportunity to add another valuable page to his reply.

His remark that I have represented the "tips of the shoulder in close anatomical connection with the *side of the middle of the neck*" is too ridiculous to be seriously meant. Or, has really Dr. Shufeldt overlooked that the mesial line is designated by a double line indicating the skin which is left in position on the right side of the body, while the single line to the extreme right represents the contour of the neck? Surely, Dr. Shufeldt is right in the last paragraph of his letter in exclaiming "let us, gentlemen, have intelligent drawings," but allow me to supplement it by praying: "Let us also have intelligent readers!"

I hardly know how to characterize Dr. Shufeldt's remark that I have represented the *biceps* muscle as "inserted into the *extensor metacarpi radialis longus*," etc. In view of this extraordinary statement I shall have to modify my above prayer somewhat, and say: "let us have *moderately* intelligent readers, at least!" or "Let us have readers who are willing to open their eyes!" Anybody with eyes and willing to see, will find upon examining my fig. 2, that the muscular slip which "is inserted into the *c. m. r. l.*, between the *tenso patagii brevis* and the humerus" is not lettered *b.* but the muscle lying behind it and partly concealed by it! The tendon to which Dr. Shufeldt refers is not lettered at all!

The above may be sufficient to lay the dust. Aside from the consideration that his criticisms of my drawings are unfounded, to say the least, Dr. Shufeldt ought to have carefully avoided any allusion to unintelligent drawings,—for he who lives in a glass house should not indulge in throwing stones, according to an old adage, the soundness of which may be indisputable even in New Mexico,—as will be perfectly demonstrated by the following interesting reflections. When Dr. Shufeldt made the figures to accompany his first paper ('Science,' June 24, 1887, figs. on p. 624) he still labored under the impression that *Rhamphastos* was figured by Garrod as the type of a passerine bird ("Garrod chose the wing of *Rhamphastos cuvieri* to illustrate the arrangement of the patagial muscles in the Passeres"). He copied this figure (fig. 1) and accordingly inscribed it ("... left wing of a passerine bird, *Rhamphastos cuvieri*"...). He then drew the arm muscles of a Swallow (fig. 2) to match, showing his own discovery; but believing the *Rhamphastos* to be one of the Passeres he fell into the—to an avian anatomist—most unpardonable blunder

of representing the Swallows as having the *propatagialis brevis* inserted in the same way as the *Rhamphastos*, in other words, after the fashion of the picarian birds. Whether that drawing was sent to 'Science' by a mistake, or not, is of no consequence; the fact remains that a man, who is going to teach others all about the "taxonomic muscles" in birds, has prepared such a drawing and finished it so far that it could be reproduced by the regular photo-engraving process. I approve most heartily of Dr. Shufeldt's concluding sentence: *Yes, let us by all means have intelligent drawings!!*

Finally a few words in regard to the name of the much talked of muscular slip.

The only *rational* name of it is the one given by Fürbringer, viz., *pars propatagialis musculi cucullaris*. This is evidently an instance "where the name is five times as big as the muscle," which, "for the sheer sake of clearness and convenience." Dr. Shufeldt wants to lay aside as an abominable name bestowed by the "old anatomists." Here Dr. Shufeldt again proves his ignorance of Dr. Fürbringer and his works. Fürbringer is not one of the "old anatomists," he is one of the younger ones, and he is, moreover, the great reformer of myological nomenclature "for the sheer sake of clearness and convenience." The name given by him signifies that this muscle is only a patagial slip of *musculus cucullaris*, leaving nothing to be desired in regard to clearness and convenience, for, of course, in speaking of it Fürbringer does not use the whole name, but simply "*propatagialis cucullaris*," which is hardly longer than Dr. Shufeldt's "*dermo-tensor patagii*." The latter, however, is neither clear nor convenient, for surely *propatagialis longus* is the true *dermo-tensor patagii*, and not the slip of *cucullaris*, which in most cases is only a *dermo-tensor parapatagii*.

Washington, D. C., December, 1887.

LEONHARD STEJNEGER.

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## NOTES AND NEWS.

IN THE last number of the 'The Auk' (Vol. IV, p. 359) reference was made to the movement for the erection of a monument to John James Audubon in Trinity Cemetery, New York City. The movement has now become well organized, under the lead of a committee of the New York Academy of Sciences, consisting of Prof. Thomas Egleston of the School of Mines, Chairman, Dr. N. L. Britton of Columbia College, Secretary and Treasurer, and Prof. Daniel S. Martin of Rutgers Female College. As already stated (see p. 97 of this issue), a committee to cooperate with the committee of the New York Academy was appointed by the American Ornithologists' Union at its late meeting in Boston, consisting